THE ROCKEFELLER INSTITUTE FOR MEDICAL RESEARCH

66TH STREET AND YORK AVENUE
NEW YORK 21, N.Y.

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Dear Josh:

I have enclosed a draft of a manuscript for your perusal, criticism addition to and so forth. I thought it was written in a straight forward expositional style but people have again claimed it takes too much for granted from the reader, I've sent it to you anyway so you might see the kind of development and the data in its totality.

Notes to your notes:

1) In doing comparative adsorption as you describe it, what would be measured, certainly not the rate as to do this adsorbant must be in excess.

2) Tracer experiment, if done, would be to show that PLT-22 was an injecting phage. Accepting phage = FA from other data would make them brothers under the skin.

3) Believe I understand your lysogenesis- protection experiment. Shouldn't you get more transductions after the lysis due to transduction by the lytic phage of the lysogenic survivors, in fact so many more that it would be difficult to say which phage did the transduction, especially the 22.

4) The only data I have on the adsorption by non-XII 2 is that 10^{10} cells of S.typhi T2 adsorbed 80 % FA (10^{3}) as contrasted to 98 % by 901. No phage data were obtained. Have no data on other transducing phages.

5) re blocking with anti-bacterial serum: Serum to SW-414 did not effectively block transduction even when cells were agglutinated (1/10). Transinductions were late in coming up and pinched in appearance but negligable loss from expectation

6) Would appreciate your verification of the following: My 22V has o.7 the U.V. inactivation slope of 22. Were done separately at first but since phage impure and any dose reduction possible, I repeated with mixed lysates and scored simultaneously.

7) SW-435 has reverted every now and again but usually from a well transfered slant. By starting from a single colony again stabilizes.

8) Differential centrifugation as suggested is too much trouble as I don't have an ultra-centrifuge available and would involve much preliminary juggling. I can occassionally sneak in a preparatory run but that is all. Believe my data amount to a differential centrifugation since all of the FA-phage was not sedimented and remained in constant ratio in the supernatant.

Point for some serious discussion: Your attempt to make transduction a generic term is not going over at all. In principle I agree with you but practically it won't work. Transduction has come to mean that which happens in Salmonella as contrasted to transformation in pneumococcus etc. I have spoken to

numerous people both interested asnd disinterested and although they all agreed on the necessity of a new word for Salmonella (some not caring for the word in particular) all resisted any attempt to call transformation a transduction, in any sense. The general attitude is that the experimental situation may eventually allow for the calling of transduction, a phage mediated transformation, but not vice versa. We discussed and disagreed about this when you used transduction for the ambulatory F factor in coli and your use has caused some confusion. I agree the erudite won't be confused but the adudience is wider than that.

The problem came up specifically in selecting a title for the CSH Symp. If I used both transformation and transduction in the title I would have directly contradicted you which considering the origin of transduction would have been awkward. To just use transduction and intend to say anything about transformation sent Hotchkiss in a tizzy (he will probably aid in the writing and discussion and although having talked himself out of a more direct presentation). I chose a neutral title thus avoiding for the time being the situation. This may all seem like a lot of mess an fuss for nothing but everyone is so word conscious these days. Would you agree to dropping all other meanings of transduction and defining it operationally?

Sincerely,

Norton